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ON THE MOLECULAR CHANGES THAT ACCOMPANY THE MAGNETISATION OF IRON, NICKEL, AND COBALT.*

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In a paper read last year, by the author, at the British Association Meeting, at Bradford, it was shown that certain molecular phenomena known to attend the magnetisation of iron were found to accompany the magnetisation of nickel and cobalt. Notably this was the case with the peculiar sound emitted on magnetising and demagnetising these metals; with cobalt the note was clear and metallic, and louder than in the case of iron. The physical as well as the chemical properties of the three magnetic metals were, in every case, proved to be so closely similar that it was reasonable to expect that any cause producing a molecular change in the one metal would produce a similar change in the other.

Fig. 1.

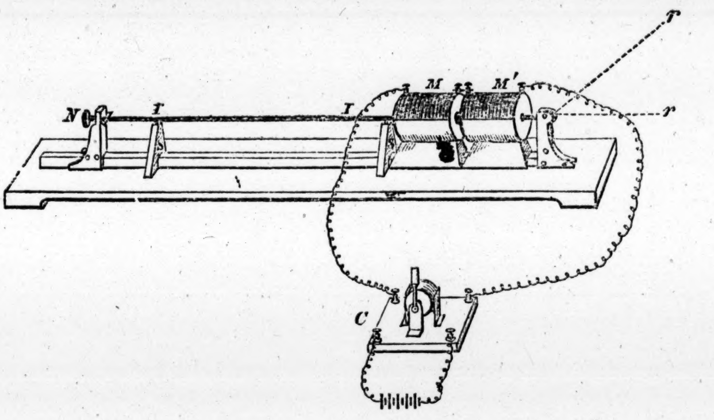
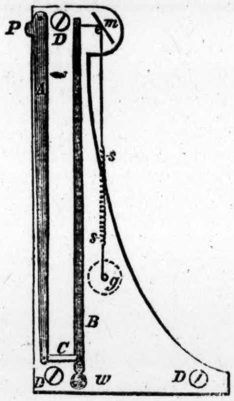


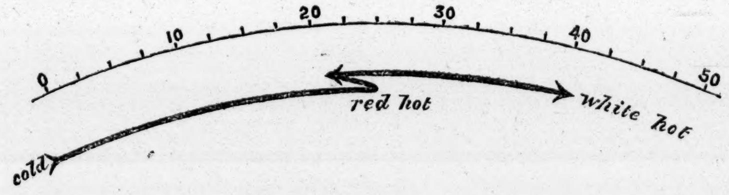
Fig. 2.



A series of preliminary experiments established the fact that a change in the dimensions of nickel and cobalt occurred on their magnetisation by an electric current, corresponding to the elongation and retraction of iron investigated by Dr. Joule. To pursue this enquiry further a committee was appointed, consisting of Professor Balfour Stewart, and subsequently Professor Clerk Maxwell, in conjunction with the author: after the trial of various arrangements an instrument was devised, and has been constructed by Messrs. Yeates, of Dublin and London, by which it is hoped precise determinations may shortly be made (Figs. 1 and 2).

Associated with these molecular changes are others relating to the heating and cooling of wires of the magnetic metals. Mr. Gore discovered that an iron wire, when raised to a white heat, underwent an anomalous expansion at a certain point during its cooling. On repeating Mr. Gore's experiments, the author found a similar anomalous deportment during the *heating* of iron wire,—the wire momentarily contracting when the temperature of a dull red heat was reached, and, as far as the eye could judge, this was also the temperature at which the elongation occurred in cooling (Figs. 3 and 4). Examining the wire during the experiment, in a darkened room, another remarkable phenomenon was revealed, viz.;

Fig. 3.



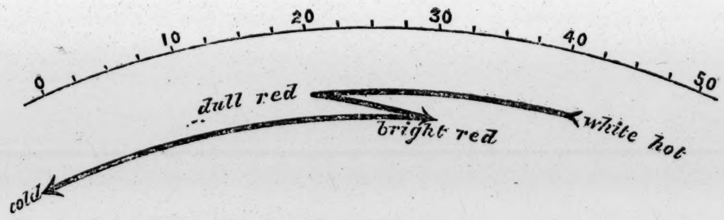


Fig. 4.

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* Abstract of a report read before Section A of the British Association, at Belfast.

that at the critical point during the *cooling* of the wire a sudden glow diffused itself over the wire, whereby it was raised from a dull to a bright red heat. During the heating of the wire—whether by an electric current or by a row of gas-flames—a momentary cessation of heating occurred at the critical point. Enclosing the wire in an air thermometer, these effects were made more evident,—the expansion of the air during the heating of the wire ceasing for a moment, when a dull red heat was attained; and on cooling, the after-glow caused a momentary and violent depression of the liquid index (Fig. 5). A crepitation in the wire was heard at the moment this change took place. Moreover, the critical temperature was approximately that at which iron lost its magnetic character upon heating, or regained it on cooling. Further, Prof. Tait has shown that at this temperature the thermo-electric condition of iron changes, and that at a lower temperature nickel undergoes a similar change. Hence it is most probable that nickel and cobalt will be found subject to analogous molecular changes at the temperature wherein their transition from the magnetic state takes place,—nickel at a lower temperature than iron, and cobalt at a higher.

Fig. 5.

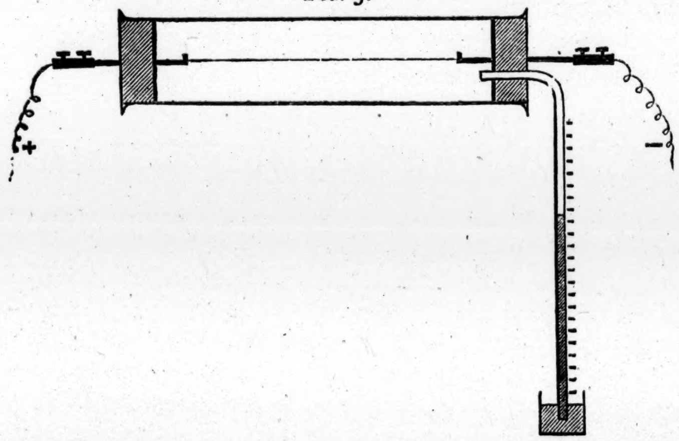


Fig. 6.

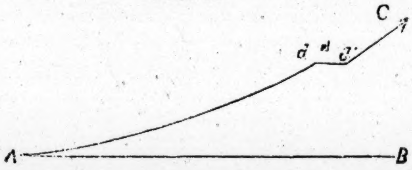
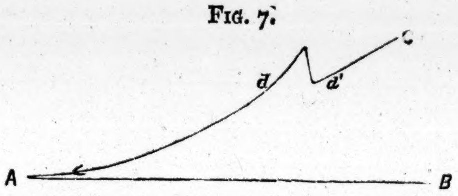


Fig. 7.



Experiments have also been made to exclude the effect of oxidation during the heating of the wires. Iron and steel wires have been enclosed in atmospheres of hydrogen, nitrogen, and carbonic acid gas, and in each case similar effects are produced to those already described. In carbonic acid the gas is decomposed around the heated wire, and brilliant scales, apparently of graphite, are deposited on the iron.

The foregoing remarks the author begged to be regarded as merely fragmentary observations, preliminary to a more detailed investigation which he hoped shortly to undertake.

After Prof. Barrett's paper had been read the following discussion took place; through the kindness of Prof. Everett, who took short-hand notes, we are able to place the substance of it before our readers:—

Professor Clerk Maxwell said—In theoretical investigations an assumption is often made that if you make a slight change in the circumstances a slight change will be produced in the result; but this is not always the case in Nature. It seems that in the cooling of iron wire, described by Mr. Barrett, something goes off at a certain moment—something tumbles over at the moment of the kick. Other things tumbled over too,—traps for catching mice, for instance,—and the parts might not have the power of getting up again. If a row of bricks were placed on end, and the first one knocked down, it would knock down the next, and so the whole row would tumble down. When I was a boy we used to call this "Sending Jack for mustard." This mode of the transference of motion was not the same thing as wave motion, because in a wave the particles swing back, but the bricks have to get up again how they can, and won't return to their former position automatically. So in Prof. Barrett's experiment, a critical point is reached where a break-down occurs among the molecules. In the process of cooling iron wire something happens which makes a noise, makes the wire lengthen, and makes it glow again. A slight change in the circumstances has made a great change in the phenomena. Seeing Mr. Herbert Spencer present reminds me he has written that something of the same kind may take place among the nerves, though I do not agree with the assumption he has made.

Professor Guthrie—Having had the opportunity of seeing Mr. Barrett's experiments, I think Mr. Barrett remarked the re-heating passed along the wire in a definite direction; the glow did not appear immediately all over, but travelled along the wire.

Professor G. C. Foster—The wire is not likely to rise equally in temperature, or to fall equally, through the whole wire. The ends are fastened to masses which tend to cool them quicker than the central parts of the wire. Is there evidence of anything more than this in the phenomenon described?

Professor Barrett—It is the fact, as Professor Guthrie has mentioned, that the "after-glow"—as Professor Tait has called it—does not appear simultaneously throughout the wire. A wave of heat seems to travel along the wire, and the sudden rise of temperature and measured progress of this wave are very beautiful to observe. I do not think the existence of this wave is due—at any rate not wholly due—to the unequal distribution of heat caused by the masses of metal to which the wire is attached. For, in reply to Prof. Foster's question, I may remark that if an iron wire, such as that tying down the cork of a soda-water bottle, be coiled round a pencil, and the helix of wire thus formed heated in some dark corner in a Bunsen gas flame, on the withdrawal of the wire from the flame the after-glow will be readily seen, generally traversing the wire in the manner already described. I am inclined to think the existence of this wave may be due either to an inequality in the diameter of the wire, to a want of homogeneity in its structure, or to the accidental passage of air-currents; for the progress of this wave is sometimes in one direction and sometimes the opposite.

Mr. HERBERT SPENCER—Is it the fact that the wire having once undergone this transformation, when it passes to a lower temperature, and is again heated, it will undergo a similar change?

Professor BARRETT—This is the case. The after-glow can be made to appear a dozen or more times upon the same wire. But the phenomenon becomes fainter each time. The gradual extinction of the after-glow which I have thus obtained seems to depend very much on the degree of tension to which the wire has been subjected during the heating and cooling, but this is a point I have reserved for future experiments, and cannot therefore speak positively upon it now.

Professor EVERETT—Is there a want of symmetry between the sudden change which occurs during the heating and that which occurs during the cooling? Would Professor Barrett be good enough to draw the temperature curves for both?

Professor BARRETT—I am much obliged to Professor Everett for his suggestion. The following curve will approximately represent the progressive temperatures of the iron wire during its heating: where A B, in Fig. 6, represents the time the wire is exposed to the source of heat, and A C the increment of temperature, judged by the eye or an air thermometer. A pause in heating, *d d'*, occurs at a dull red heat. On the other hand, the cooling of the wire may be represented as in Figure 7. Here there are two points, *d d'*, at different intervals of time in cooling, where, nevertheless, the temperature of the wire is the same. The curves, therefore, are not symmetrical. Of course the continued accession of heat in Fig. 6 would mask any small fall in temperature if such occurred. In both cases the anomalous temperature is seen only as the result of a differential action.

After a remark by Professor Clerk Maxwell on the analogy of these phenomena with Mr. Spencer's statement on the propagation of nerve force already referred to,—

Mr. HERBERT SPENCER replied—I rather think Mr. Barrett's experiments confirm my hypothesis. When the nerve impulse has been expended, I infer that the nerves are reinforced from the neighbouring tissues. They fall in temperature, and instantly absorb heat from adjacent tissues. As a verification of this inference, I may mention we have evidence that the velocity of nervous transmission is variable according to the temperature of the body—the current being swifter in summer than in winter, which corresponds with the fact that the "personal equation" is a variable element in different individuals, and may be presumed to vary in consequence of their slight difference of temperature.

Professor MAXWELL—I attribute to Mr. Spencer the view that the nerve recovers its power of acting by absorbing heat, The people who read such a statement might attribute to him the notion that the nerve acts as a heat-engine. It may be that when the nerve is raised to a higher temperature, some chemical action may be able to take place which, though it is a lowering of the available energy of the system on the whole, may make it more available for giving another impulse—another wave of nerve force—but at the same time, after all is done, the nerve is worse off than before, so that it has to receive from the blood something to bring it up to its former state. The two sides of the interpretation are these: We all admit that nerves won't go on without food, but it may be possible that some action depending upon the absorption of heat from the surrounding tissues, may make the nerve more instantaneously available. But those who believe in thermo-dynamics, and the dissipation of energy, cannot admit that any purely material system can convert heat into work when the system and its whole environment are, at the commencement of the operation, at the same temperature. There is a statement in Sir W. Grove's address on continuity, in which he mentions that Berthelot had discovered that a certain salt could undergo chemical change in which its energy was increased by absorption of heat from the surrounding medium, at a temperature no higher than its own. That statement has not been attacked; but. though it supports Mr. Spencer's view, it cannot be understood literally by anyone who believes in Carnot's law, which expresses under what conditions heat can do work in passing from one thing to another.

Mr. HERBERT SPENCER—I by no means assume that there is any genesis of force made possible in a nerve by any such absorption of heat. Although not very familiar with thermo-dynamics, I am sufficiently so to be aware that there is no possibility of such an evolution of force in the nerve except at the expense of the body at large. I am merely endeavouring to show that we have in the heat of the tissues surrounding the nerve a reserve of force ever present, which may be transformed into the force that traverses a nerve simply by the instrumentality of the change in the isomeric forms of the molecules of nerve; which change is a change of a kind permitting them, each time they absorb heat, to assume a less stable form, and therefore again to fall into the more stable form. It is a perfectly reasonable hypothesis that a nerve may be composed of material which, like all the other forms of protein, admits of extremely easy change from one isomeric state to another. If it is supposed that by the absorption of a small amount of heat, the molecules of the nerve assume such a relation that by a slight disturbance they will again resume their more stable state, and send a cumulative wave of disturbance—like Professor Maxwell's line of bricks:—I say it is a feasible assumption that when they have done that, they have lost heat to the extent that they have given out that wave of motion, and are in a condition to absorb heat from the tissues. That the ability of the nerve to perform its function depends on absorption of heat from the surrounding tissues is a supposition confirmed by the fact that if you reduce the temperature greatly, the nerve ceases to act.

Professor CLERK MAXWELL—No doubt there is plenty of power in the heat of the body to renovate a nerve. But as heat can do work only by passing from a hotter body to a colder, "all the king's horses, and all the king's men," could not renovate the nerve by the aid of heat alone.

Here, we regret to say, the discussion was brought to a close so far as the public were concerned, owing to the press of other papers in hand.

THE AMERICAN ELECTRICAL SOCIETY.

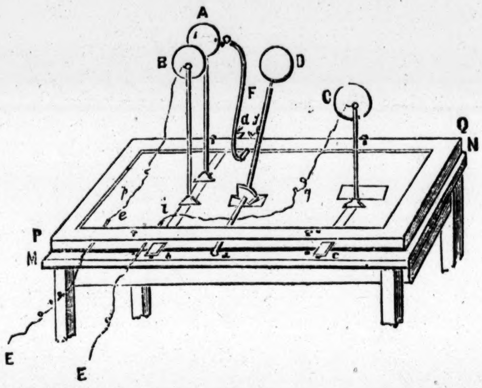
THE good which has been done by the English Society of Telegraph Engineers during the short period of its existence—in promoting the general advancement of electrical and telegraphic science, as well as in diffusing a knowledge of the most recent improvements and ideas amongst the members of the profession—is universally acknowledged. Soon after its establishment the want of a similar institution began to make itself felt in the United States, and this, combined with the success which has already attended the efforts of the Society of Telegraphic Engineers, has led to the formation of a Society in that country under the name of "The American Electrical Society." Chicago is for the present to be its meeting-place, and General Anson Stager, of that city, has been elected the first President. Amongst the Vice-Presidents—seven in number—are Mr. Orton, the President, and Mr. Prescott, the Electrician, of The Western Union Telegraph Company; Mr. Haskins, the General Superintendent of the North-Western Telegraph Company; and several other names well known in connection with American Telegraphy. The constitution is evidently modelled after that of the English Society:—"The object," it is stated "shall be the interchange of knowledge, and the professional improvement of the members; the advancement of electrical and telegraphic science, and the establishment of a central point of reference." Among the means to be employed by the Society for attaining these ends shall be "Periodical meetings for the reading of professional papers and the discussion of scientific subjects; the foundation of a library; the collection of electrical instruments, models, drawings, maps, &c.; and the publication of such part of the Proceedings as may be deemed expedient." The meetings are to be held on the third Wednesday of every month, and the condition of membership is that candidates after being elected—"by receiving at least three-fourths of the votes cast"—shall pay an entrance-fee of five dollars and an annual subscription of two dollars.

The good which may result from the establishment of similar Societies amongst the civilised nations of the world where Telegraphy has been established as a system, provided only they are properly conducted, can hardly be over-estimated. The facilities which they afford—not only for the interchange of ideas, but for bringing to light, as well, what might otherwise have remained hid under a bushel—cannot fail to be productive of the best results for the future of Telegraphic Science, and we shall watch with interest the career of this, the latest addition to their number.

ON THE MEDIUM IN ELECTRIC INDUCTION.

By Dr. H. BRONGERSMA.

FARADAY propounded a theory of electric induction in which he not only assigns an essential part to the substance intervening between the electrified and the induced conductor, but considers it as a medium, in which the electric action is propagated from molecule to molecule. This theory has found few adherents. There is a tolerably general conviction that it has been refuted by an experiment of Riess. Dr. Brongersma, after having occupied himself with an experiment on the electric condition of non-conducting bodies, Riess's experiment appeared to him not quite unobjectionable. Hence he resolved to enter anew upon an experimental investigation, the results of which we now submit to the consideration of our readers. After a detailed exposition of Faraday's theory, for which we must refer to the "Experimental Researches" (1669, 1670, &c.), and of the objections raised against it, the author proceeds to give an account of his own experiments.



He commenced with apparatus similar to that used by Riess, but in order to obtain greater accuracy he adopted the following arrangement:—The figure shows a wooden table, 1·00 metre long and 0·04 metre broad. The plate *q* rests by means of four set-screws upon a frame, M N, supported on four feet. The projecting edge of the table is provided with a deep channel, in which fits a glass case, 1 metre long, 0·76 metre broad, and 0·76 metre high, all superfluous room being filled up with mercury. This case consists of five plates of glass, of a good isolating quality, fastened together hermetically at their edges. Two copper disks, A and B, 112 m.m. in diameter, rest upon vertical rods of ebonite, 11 m.m. in diameter, whose pedestals are fastened to wooden pushers, which project, fitting tightly, through the edge of the table. By means of the knobs *a* and *b*, fixed to the pushers, upon which there is a scale graduated in millimetres, the mutual distance of the disks can be regulated at will, whilst they remain always parallel. The distance of the central points of these disks from the surface of the table is 0·4 metre, and from the nearest glass side-plate 0·22 metre. The surfaces of the copper disks facing each other are not varnished. A third copper disk rests also upon an insulating rod, whose foot is movable, in a similar manner, by means of the knob *c*, so that the surfaces B and C may always lie in the same vertical plane. The centre of the disk C is 0·215 metre above the surface of the table, and 0·16 metre from the nearest side-plate. Both disks B and C are connected by means of thin copper wires, which are carried through the edge of the table with due insolation, with the two electroscopes E and E′ fitted up with a dry column according to Fechner's arrangement. Both electroscopes are placed upon a table, the one partly in front of the other, E′—which is connected with C—being nearest to the observer. The axle, *d d′*, of hard wood fits tightly through the margin of the table, and bears a sector-shaped pedestal, upon which is fixed an ebonite rod vertical to the direction of the axle, so that, by means of the knob *d*, it can be moved in a vertical plane, parallel with the surfaces of the copper disks. Upon this ebonite rod the disk D, whose specific inductive power is under examination, is fixed so that it can be readily removed and replaced by another. The ebonite rod is for this purpose hollowed out cylindrically to the depth of about 2 c.m., whilst the disks under examination are fitted with glass or ebonite handles, which fit accurately in this socket.

To the glass disks, glass handles are cemented with shellac, to which a little wax had been added. The disk D can be allowed to occupy any desired position between A and D, only the surfaces must be parallel and the centres must lie in one right line. If the knob *d* is turned in the opposite direction, D is placed in a similar position with regard to C, as it was before with respect to B. Near C an aperture is made in the table, which can be closed hermetically. This serves to remove the disk D, and replace it with another. Afterwards a copper rod, F, is passed through the edge of the table at *f*, having copper knobs at each end. The part which lies in the edge of the table is surrounded with an ebonite casing, firmly united to the rod, and turning in a cylindrical capsule. The rod is so bent that it can be turned by means of a handle of ebonite at *f*, till the one end is brought into conductive connection with the disk A. It thus becomes possible to charge the disk A without removing the glass case. Finally, straps of leather are attached to the projecting margins of the glass casing, as handles, by means of which it becomes possible to lift the case, and to insert beneath it a couple of wooden rods, and thus remove it. The experiments were made at first without covering the apparatus with the glass case. The temperature and the degree of moisture, as observed by a hair hygrometer, were taken into account. The disks A, B, and C received the exact position described, whilst D was placed as far as possible from A and B. For every experiment they were freed from dust by means of a hair pencil. The knob *f* is brought in connection with A, and then with the charged cover of the electrophorus. Immediately afterwards the connection with A is broken, the knob *f* touched with a conductor, and the rod F turned down on the surface of the table. In this manner A is charged with positive electricity, as assumed in all the following experiments, whilst B is kept connected with the earth by means of the thread *p*. The disks D and C are then connected with the electroscopes E and E′ by means of the wires *p* and *q*, and the condition of the disk D is then examined by bringing it near to C. If not neutral it is discharged by means of the flame of an alcohol lamp, so that for some time after it might have no influence upon the disk C, if moved in its vicinity. The disk D cannot be discharged in this manner without first discharging A, unless a metallic screen in conductive connection with the earth is interposed between them. The disk D is then, as rapidly as possible, introduced into the space between A and B, and the movement of the gold leaf in the electroscope observed. Immediately afterwards it is turned back, so as to stand in the same position to C as it has just done with regard to B, the position of the gold leaf in the electroscope E′ being observed. For this alternate movement of he disk D scarcely two seconds are requisite. It is now again placed in the space between A and B, and the experiment is repeated from time to time, noting the time that has elapsed from the first observation. The disk C is always connected with the more sensitive of the two electroscopes, the movement of the gold leaf being observed by means of a telescope, so adapted that the image of the gold leaf coincides with the image of a graduated scale. In this manner it would have been possible to determine the specific inductive power of the substance from the magnitude of the deviation of the gold leaf,—at least if the disk A was kept equally strongly charged. It may be subsequently of importance to ascertain the specific inductive powers of different media. At first the author found it merely necessary to show that there was really a difference in the specific inductive power of different media which must be taken into account in the observed phenomena of the action of electric influence.

The experiments were then repeated as follows:—The apparatus was covered with the glass case, and the air dried with sulphuric acid, and chloride of calcium placed upon the table in glass capsules. The chloride of calcium was scarcely affected in a perceptible degree even after the lapse of several weeks, whence it follows that the interior of the apparatus was sufficiently shut off from the outer air. The wood always contains moist portions; in consequence of this, even when well polished, it is impossible to keep the air in the apparatus absolutely dry. The results are given in the following tables. Column 1 shows whether the apparatus was covered with the glass case or not. Column 2 gives the time from the instant when the neutral non-conducting disk is interposed between A and B. Column 3 gives the temperature according to the centigrade scale. Column 4, the state of the hair hygrometer at the times signified in column 1. Column 5 shows whether the interposition of D in the space between A and B increased the induced electricity on B, decreased it, left it unchanged, or, if this was not the case, with what kind of electricity it was charged. The distance between A and B was 36 m.m.; that from the centre of D to A, 24 m.m., and to B 12 m.m.

A.

Shellac disk; diameter 120 m.m.; thickness, 2 m.m

1.	2. Time.	3, Therm.	4. Hygr.	5. Induced Electricity on B.	6. Electric Condition of Disk D.
Damp air	0 m.	19° C	39°	Increased	Neutral
,,	5 ,,	,,	,,	,,	Positive
Dry air	0 ,,	15½°	7½°	,,	Neutral
,,	90 ,,	,,	,,	,,	Positive
,,	4 ,,	18°	8°	,,	,,

The experiment was repeated with the same disk. but with this modification—that the disk D was placed exactly in the middle between A and B.

B.

Shellac disk; diameter, 120 m.m.; thickness, 2 m.m.

Damp air	0 m.	24° C.	35°	Increased	Neutral
,,	2 ,,	,,	,,	,,	Negative
,,	15 ,,	,,	,,	Dec.—Inc.	,,
,,	20 ,,	,,	,,	Decreased	,,
Dry air	0 ,,	15°	15½°	Increased	Neutral
,,	50 ,,	16°	15°	,,	Negative
,,	3 ,,	,,	,,	,,	,,
,,	4 ,,	15°	,,	Dec.—Inc.	
,,	5½ ,,	,,	,,	Decreased	

These tables give a sufficient view of the course of the experiments. After the disk was placed first between A and B, it was at once turned back, in order to examine its state by means of the disk C, and was then immediately restored to its former position. These experiments were often repeated at first until the disk had become negative; afterwards about every three minutes. In dry air the experiment was repeated every five minutes From the first experiment it was seen approximately when the disk D had become electric, and also when it occasioned a diminution of the induced electricity upon B. The same disk was then once more examined in the same manner, but with this difference, that it was left as long as possible. The experiment was often repeated only after the points of time in which, judging from the first experiment, a change in the condition of the disk D, or a decrease of the induced electricity on B, was to be expected. From the table A it appears that the disk D became electro-positive when the distance from the disk A was 24 m.m., and that from B 12 m.m. If the apparatus was not covered with the glass cass, this took place after five minutes. In dry air it remained neutral for an hour and a half. As a matter of course it must thereby occasion an increase of the induced electricity upon B, so that a continuation of the experiment was unnecessary. In the following experiments the disks under examination were placed half-way between A and B, the respective distance being 36 m.m. After some time it appeared that when approached to C they were become negative, since the gold leaf of the electroscope E′ approached the positive pole first, although to a scarcely perceptible extent as seen with the telescope; but the deviation increased by degrees. According to table B the disk in damp air became electro-negative in two minutes. Although the disk was negative, yet when introduced into the space between A and B it occasioned an increase of the induced electricity upon B. The gold leaf deviated towards the negative pole, whilst an opposite movement was perceptible when the disk was removed. These deviations became gradually smaller, until, at a certain instant, they could no longer be recognised. Then the influence of the negative electricity began to preponderate upon the disk A, whilst the charge of A simultaneously diminished, until the disk—when interposed between A and B—occasioned a decrease of the induced electricity upon B. In damp air this occurred after the lapse of twenty minutes; in dry air not until more than five hours. When the disk under examination had become strongly electro-negative, on approaching it to the space between A and B, where only a portion of the disk was interposed, a decrease of the induced electricity upon B was observed; but when, on moving the disk further, a larger portion was introduced into that space, there ensued an increase. The movement of the gold leaf of the electroscope E′ towards the + pole was at first smaller, then became equal to, and finally larger than, the opposite motion which ensued immediately after, until subsequently a movement towards the positive pole was alone perceptible, showing a decrease of the induced electricity. This vibration of the gold leaf, in which both deviations were almost equally great, was perceived in dry air after four hours, as appears from table B.

The rod upon which was placed the disk under examination was always electro-negative if the disk, as usual, was placed between A and B. Hereby, therefore, there can never be occasioned an increase of the induced electricity upon B. A decrease is more probable. In damp air a silk thread coated with shellac insulates best, but this cannot be used in the apparatus when covered. The author therefore examined a number of varnished glass tubes, but none of them could be made sufficiently insulating. If a glass tube was overlaid with a rather thick layer of shellac, the latter became full of chinks on exposure to dry air, so that its insulating power was much reduced. Glass rods covered with shellac insulated at first better than such as were varnished, but on the gentlest contact with an electric body it appeared that they were electric. A glass tube coated with paraffin proved no better than those covered with varnish, and a rod of shellac was too brittle for use. A rod of ebonite proved best, especially if repeatedly treated with common magnesia and sulphide of carbon. The following table contains the results:

C.

Shellac disk; diameter 80 m.m.; thickness, 2 m.m.

1.	2. Time.	3. Therm.	4. Hygr.	5. Induced Electricity on B.	6. Electric Condition of Disk D.
Damp air	0 m.	23° C.	35°	Increased	Neutral
,,	1½ ,,	,,	,,	,,	Negative
,,	15 ,,	,,	,,	Decreased	,,
Dry air	0 ,,	20°	10°	Increased	Neutral
,,	40 ,,	,,	,,	,,	Negative
,,	7 ,,	,,	,,	Decreased	,,

D.

Paraffin disk; diam., 156 m.m.; thickness, 10 m.m.

Damp air	0 m.	19° C.	39°	Increased	Neutral
,,	2 ,,	,,	,,	,,	Negative
,,	95 ,,	,,	,,	Dec.—Inc.	,,
,,	120 ,,	,,	,,	Decreased	,,
Dry air	0 ,,	15½°	9½°	Increased	Neutral
,,	30 ,,	16°	10°	,,	Negative
,,	4 h.	17°	11°	,,	,,
,,	6 ,,	,,	,,	Dec.—Inc.	,,
,,	6 h. 40 m.	,,	,,	Decreased	,,

E.

Paraffin disk; diam., 112 m.m.; thickness, 12 m.m

Damp air	0 m.	24° C.	35°	Increased	Neutral
,,	1½ ,,	,,	,,	,,	Negative
,,	105 ,,	,,	,,	Dec.—Inc.	,,
,,	135 ,,	,,	,,	Decreased	,,
Dry air	0 ,,	24½°	13°	Increased	Neutral
,,	25 ,,	,,	,,	,,	Negative
,,	6 h.	17°	12°	Dec.—Inc.	,,
,,	6 h. 30 m.	,,	,,	Decreased	,,

F.

Paraffin disk; diam. 150 m.m.; thickness, 5 m.m.

Damp air	0 m.	24° C.	35°	Increased	Neutral
,,	1½ ,,	,,	,,	,,	Negative
,,	85 ,,	,,	,,	Dec.—Inc.	,,
,,	100 ,,	,,	,,	Decreased	,,
Dry air	0 ,,	18°	9°	Increased	Neutral
,,	20 ,,	,,	,,	,,	Negative
,,	5 h. 30 m.	,,	,,	Decreased	,,

G.

Ebonite disk; diam. 154 m.m.; thickness, 2 m.m.

1.	2. Time.	3. Therm.	4. Hygr.	5. Induced Electricity on B.	6. Electric Condition of Disk D.
Damp air	0 m.	24° C.	36°	Increased	Neutral
„	½ „	„	„	„	Negative
„	3 „	„	„	Decreased	„
Dry air	0 „	14½°	8°	Increased	Neutral
„	6 „	„	„	„	Negative
„	1 h.	15°	9°	Dec.—Inc.	„
„	2 h. 30 m.	„	„	Decreased	„

H.

Sulphur disk; diam. 165 m.m.; thickness, 3 m.m.

Damp air	0 m.	23° C.	35°	Increased	Neutral
„	1 „	„	„	„	Negative
„	21 „	„	„	Decreased	„
Dry air	0 „	21°	11°	Increased	Neutral
„	20 „	„	„	„	Negative
„	3 h.	„	„	Decreased	„

J.

Glass disk; diameter, 50 m.m.; thickness, 1⅔ m.m.

Damp air	0 m.	18½° C.	40°	Unchanged	Neutral
„	1 „	„	„	Decreased	Negative
Dry air	0 „	18°	10½°	Increased	Neutral
„	1 „	„	„	Unchanged	Negative
„	1½ „	„	„	Decreased	„

K.

Glass disk; diameter, 80 m.m.; thickness, 1½ m.m.

Damp air	0 m.	24° C.	34°	Increased	Neutral
„	½ „	„	„	Dec.—Inc.	Negative
„	10 „	„	„	Decreased	„
Dry air	0 „	15°	8°	Increased	Neutral
„	½ „	„	„	„	Negative
„	8 „	„	„	Dec.—Inc.	„
„	18 „	„	„	Decreased	„

L.

Glass disk; diameter, 110 m.m.; thickness, 1½ m.m.

Damp air	0 m.	24° C.	36°	Increased	Neutral
„	½ „	„	„	„	Negative
„	8 „	„	„	Decreased	„
Dry air	0 „	17°	7°	Increased	Neutral
„	½ „	„	„	„	Negative
„	50 „	„	„	Decreased	„

L'.

Varnished glass disk. (Same as L.)

Damp air	0 m.	23° C.	36°	Increased	Neutral
„	½ „	„	„	„	Negative
„	16 „	„	„	Decreased	„
Dry air	0 „	14°	7½°	Increased	Neutral
„	½ „	„	„	„	Negative
„	2 h.	„	„	Decreased	„

M.

Glass disk; diameter, 130 m.m.; thickness, 1½ m.m.

Damp air	0 m.	24° C.	34°	Dec.—Inc.	Negative
„	1 „	„	„	„ „	„
„	6 „	„	„	Decreased	„
Dry air	0 „	19°	12°	Increased	Neutral
„	½ „	„	„	„	Negative
„	25 „	„	„	Decreased	„

N.

Glass disk; diameter, 150 m.m.; thickness, 1½ m.m.

1.	2. Time.	3. Therm.	4. Hygr.	5. Induced Electricity on B.	6. Electric Condition of Disk D.
Damp air	0 m.	24° C.	34°	Decreased	Negative
Dry air	0 „	17°	12°	Increased	Neutral
„	½ „	„	„	„	Negative
„	3 „	„	„	Decreased	„

O.

Mica disk; diameter, 155 m.m.; thickness, ⅕ m.m.

Damp air	0 m.	24° C.	36°	Decreased	Negative
Dry air	0 „	18°	10°	Unaltered	„
„	4 sec.	„	„	Decreased	„

In addition to these disks others have been examined, but the author does not think it needful to multiply the tables, as the results agree perfectly with the foregoing. Only the results obtained with the glass disks I, K, L, M, and N have been given, since all these have been cut out of the same plate of glass. The experiments in undried air were undertaken in serene winter weather, in a well-warmed room,—consequently under the most favourable conditions as regards temperature and moisture. From this investigation it appears that the results depend in a great degree on the dimensions of the disk, when the conduction on the surface is good.

The author was desirous of pursuing the matter further with ebonite, but the disks which he procured contained metallic particles,—perhaps sulphide of antimony.

The vibratory motion of the gold leaf in the electroscope, on moving the disk D towards the interval between A and B, was perceived only when this disk had received an electric charge. The glass disks, K, L, M, N, and the mica disk O, occasioned this phenomenon in the first experiment. If the disk was immediately turned back, and brought to C, it proved to be moderately strongly electronegative. In dry air this vibration of the gold leaf was never perceived in the first experiment, except with a glass disk covered on the one side with tinfoil, and which, after removal of the foil, proved to be made of strongly conductive glass. This displayed in dry air the same phenomena as did the above-named disks in moist air. In case of all the other disks this was not perceived until they had been for a longer or shorter time in the space between A and B.

From the above it appears plainly that this phenomenon—in which Riess thought he could recognise a proof against Faraday's theory—is merely a consequence of the charge which the disk receives by conduction on its surface, and demonstrates nothing against the theory. Another phenomenon —*i.e.*, that some of the disks examined, even in the first experiment in damp air, occasioned no increase of the induced electricity—which, according to Riess, would be a decisive proof against Faraday's theory, must plainly be attributed to the same cause. Thus, in Riess's papers, we are not only unable to find any argument against the theory, but the views of Riess fail utterly to explain the phenomena. If, however, these phenomena are due to conduction, the disks, both of shellac and of glass, must be conductive in so high a degree that a separation, as well as a re-union of positive and negative electricity, can take place in an immeasurably brief time, whilst in the first case it requires half an hour, in the second scarcely half a minute, before an excess of negative electricity is present, although they were placed in the same conditions and upon the same isolating rods. If Riess had observed more caution in his experiments, and had taken account of their duration, he would have found in them a confirmation of Faraday's theory, and have avoided much in his later essays which must be regarded as inaccurate.

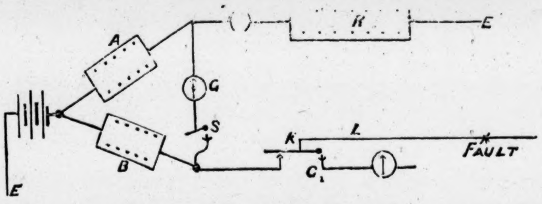
(To be continued.)

FAULTS IN SUBMARINE TELEGRAPH CABLES.*

By J. J. FAHIE.

THE author states how, about eighteen months ago, he commenced a series of experiments, with the object of discovering some method by means of which faults in submarine cables might be more accurately localised than at present. He began by endeavouring to verify the statement that "the current set up in a broken cable is always steady and positive," and for this purpose carried out a large number of experiments upon a very faulty section of the first Persian Gulf cable, which was fortunately lying at his service. From observations of these—which were made at all hours of the day and night, on four different sections of cable, and extended over several months—the author was driven to but one conclusion, and that was, that the currents in bared cables cannot be distinguished in the matter of strength, except perhaps at times, from the ordinary earth-currents to be found in all telegraph lines, good or bad, whether over or under ground. He is of opinion, however, that this is strictly true of very faulty cables only.

The author next set himself to eliminate this cable-current as far as possible. One of the methods named in Mr. Culley's "Handbook of Practical Telegraphy," viz., "the application of a strong negative current for ten or twelve hours, interrupting it occasionally, and then giving a short positive current," is, he thinks, of little value for fixing accurately the position of a fault, on the ground that is employed without any reference to the condition of the cable-current at the time of testing. Having proved that the current from a faulty (bared) cable is identical with the earth-current, and may therefore vary from positive to negative during the time the strong negative current is on the line, he proposes to substitute, for the method named above, the sending into the line a current of the opposite sign to that coming from it, and arranging the strength and duration of this current to suit the strength of the one from the cable. The cable being then allowed to discharge itself through a galvanometer, the needle will gradually approach the zero point, and, immediately this is the case, the cable is for the moment free, and the test is made. Employing Wheatstone's bridge for this purpose, the author carried out the following arrangement:—P is a circuit-breaker; when the plug is removed the connection between the branch coils A, and the resistance-box R is severed; *k* is an ordinary key for putting the line to earth through the galvanometer G', or the bridge, as may be required. The resistance of the cable is first measured by an ordinary test, and is left unplugged on R; *k* is then depressed, and the cable put to earth through an ordinary galvanometer, G, for the purpose of ascertaining the nature of the current in it. To re-polarise this current P is removed, the switch S is opened, and a current opposite in sign is sent into the cable. This is allowed to remain until, by depressing *k* at intervals, it is ascertained that the cable-current is re-polarised. As soon as this is so, *k* is depressed, P is inserted again, and, immediately the needle of the galvanometer G_1 is at the zero point, the test is taken by closing the switch S, and adjusting R in the usual way, until the needle of G remains at zero. This must be done very quickly, as the cable-current speedily sets in again, and if this is the case before the test can be made the whole process has to be repeated.



The author states that he found a positive cable-current could be more easily eliminated by a negative current than a negative cable-current by a positive current sent into the line. In the latter case not only was the fault depolarised, but the exposed wire got covered with some badly conducting substance, increasing the total resistance, and retaining the needle of G_1 for a considerable time at the zero point.

In carrying out his numerous experiments upon the subject, the author found that the statement made by Mr. Latimer Clark, as to "the resistance of a small fault in a cable being much greater with a small battery power, of say 5 cells, than with a higher power, of say 50 cells, whereas in the case of a large fault the resistance will be more nearly equal." was not correct. The different resistances obtained with different battery powers are due not to the great or small resistance of the fault, but to the presence of a current in the cable. When this was not present, or when it was entirely eliminated by the method described above, the author found that an ordinary test always gave exactly the same result, no matter what the battery power employed in testing might have been.

MR. ALEXANDER ADAMS said that, during the course of experiments which occupied some eighteen months, he was unable to get any satisfactory result by eliminating the currents of the cable to find a fault. During a long course of experiments with what were called Lumsden's tests, he came to the following conclusions:—First, that a zero point between the polarity of the cable does occur, and that it will last from two to three seconds; in the second place, that the time occupied in depolarising an exposed end is proportionate to the time occupied in polarising that end, with a given power in all cases; thirdly, that the time occupied in depolarising an exposed end is also proportionate to the area of surface exposed, time and power being equal in each case. He thought that that was a most important result. It might prove to be the cause of some of the discrepancies in that test for the probable amount of surface exposed, by the ten cells and the sixty cells. In the case of a broken cable consisting of three or four conductors, the end would be "cleaned" by a negative current. He generally used ten cells, and kept the action up for ten or fifteen minutes. He then polarised with a positive current, from his testing battery of forty or sixty cells, for one minute. Time must be maintained very exactly, or the result would be lost. If they polarised the fault in one conductor for one minute, and then reversed it to the negative pole of the battery, it would be found that the conductor which—with the same time and power—occupied the greatest time in depolarising had the largest surface exposed. That was most important, because it enabled the electrician to choose that for his test. Suppose the diagram to represent the degrees of an ordinary differential galvanometer. They first sent a ten-battery negative current, for ten or fifteen minutes; they then reversed, watch in hand, to the positive pole of the testing current, and for thirty or forty seconds, or one minute, or any fixed time, they reversed to the negative pole. If they had by previous tests brought the resistance to the proper amount, the needle would move across, and make a very slight pause at forty. Then it would go on, and make another pause of two or three seconds. It would go on again, and make another pause, and after that it would go right across very quickly, and no resistance would alter it. It would go "bang" to one side. Due precaution must be taken, in each case, as to the time of polarising with the positive pole. Now let them look at the cable end through a magnifying glass, as they followed the needle round. After cleaning the end of the cable with a negative current, they sent a positive battery current into the cable, and gradually a white substance formed round the exposed end. That pole was kept on for one minute, and then they reversed to the negative pole. The needle would gradually move. When it paused at a certain point it would be seen through the magnifier that the white substance—which was chloride of copper—had commenced to crack from the end, and gradually fell down. As it fell the needle gradually moved, and when the needle stopped it would be found that the cable was quite clean. Directly a bubble arose from the exposed end the needle began to move over, and when bubbles began to proceed from the whole of the exposed end the needle came quickly over to the other side. He had failed in a great number of instances, without being able to find out why; but he had afterwards proved that if the end was overcoated with chloride of copper, that substance would crack and fall away before he was ready, and that this would spoil the test. The object was to keep the chloride of copper intact round the exposed end, until they could bring the negative current to bear upon it; that would produce what he supposed was hydrochlorate of soda, or something of that sort. It took him eighteen months to find out the reason of the failure of the test. The method now gave him very good results.

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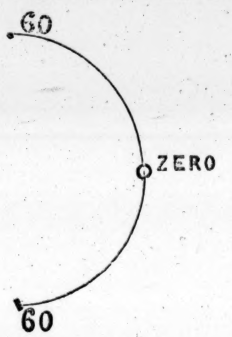
* Abstract of a paper read before the Society of Telegraph Engineers, November 11th, 1874.



Mr. Phillips said that a very important question had been started by the paper, and if only it gave the Society the advantage of having some remarks from Mr. Clark and other practical gentlemen he had no doubt they would be much obliged to Mr. Fahie. But, as regarded the paper itself, he did not think that Mr. Fahie had brought forward anything new or particularly instructive. In fact he seemed to have gone to some trouble to develop facts which he (Mr. Phillips) presumed were familiar, and which were pretty generally and fully appreciated,—at all events, by the members of this Society. For instance, Mr. Fahie had taken some pains to prove that the current from a faulty cable was not always positive and steady: there was a statement somewhat to that effect in Mr. Culley's "Handbook of Telegraphy;" but Mr. Culley there spoke of the current coming from the fault pure and simple, and he did not mix it up with earth-currents in any way. And surely one would not be justified in expecting a positive current to be always flowing from the end of a faulty cable. Mr. Fahie seemed rather to have mixed the subject up, and he did not seem to appreciate the fact that a current coming from a faulty cable would, in most cases, be due to two causes,—first, the earth-current due to a difference of potential between the two points on the earth's surface, and, secondly, the current due to the electromotive force set up by the fault itself. These two forces might either coincide or oppose each other. Mr. Fahie then seemed to come to the conclusion that it was necessary to eliminate the cable current: that, in itself, was certainly a very wise conclusion, but still he (Mr. Phillips) should fancy that almost everyone entertained that opinion. In fact, all the tests that existed endeavoured to do that, more or less, and the real question seemed to be whether Mr. Fahie did it better than it had been hitherto done. In Mr. Culley's book there was a simple statement as to the mode of testing a fault; and what did Mr. Fahie do more than was directed by Mr. Culley? Certainly Mr. Culley did not tell them to put in another galvanometer, for he probably thought that he could depend upon one galvanometer; neither did he tell them to depend upon the first rush of the current, and to do so certainly seemed to be rather a curious feature. It must be recollected that the fault which Mr. Fahie was dealing with was rather a simple case. It was evidently a very large fault which, with simple reversals managed with a little dexterity, would have given results quite as near as those obtained by Mr. Fahie.

Mr. France said that he could only endorse what Mr. Phillips had said. It seemed that the matter which Mr. Fahie had brought before them was really such a simple one that if they had faults, of that low resistance, most electricians—irrespectively of the ordinary mode of testing by the bridge—would be so nearly certain in such a case that it would be hardly worth while to try any other method. In fact, Mr. Fahie said that he obtained by that common formula 490·5 units, whereas the fault was found at 493 units. He (Mr. France) wished that they could always arrive as closely as that. Mr. Adams had spoken about Lumsden's method, and he (Mr. France) had certainly found it exceedingly correct where there was a multiple cable. It was a method which ought to be followed as much as possible by electricians. But when there was a multiple cable, his own mode of procedure was always to find the wire which gave the highest resistance, and to seal its end. By very close observations he found that the inductive capacity was about the finest test which he could possibly get for any multiple cable. If it was a single wire cable he should never dream of getting rid of the end, as it would be too valuable; but where there were several wires it was a great advantage to have the induction test to verify the resistance test.

Mr. W. H. Preece said that he thought the paper which had been read was a very proper one for the Society. In fact it fulfilled one of the objects for which the Society was instituted, which was that those members who were situated at distant places might have an opportunity of laying before their brethren in the profession facts which came within their knowledge. It could not be imagined that everybody knew everything, or that gentlemen who were situated almost on the confines of the earth, as in the hot regions of Persia, should know all that had been done upon our own shores. When they found a young man, like Mr. Fahie, devoting his time and his energies to scientific pursuits, he (Mr. Preece) thought that they ought to hail such papers as the present with great pleasure, and give the author all the support they could. The facts which he adduced were very few, but they were put forward very clearly, and among them there were one or two that were new. Mr. Fahie had committed a few errors in his paper, such as they all committed. They were errors principally in the use of terms upon which no two men were agreed. The paper spoke of different currents, at one time using the term "cable-current" when "earth-current" was meant, and sometimes using "earth-current" when "cable-current" was meant. Mr. Fahie was not the only person who made this mistake. Great confusion was introduced into all books by speaking of currents as being positive and negative, implying the existence of two separate and distinct currents. They must not forget that there was really only one current, and that this current varied only in direction. When they spoke of a positive current, and a zinc current, they really used an anachronism. In one portion of his paper Mr. Fahie alluded to the fact that, in order to overcome a positive current, which he called a "cable current," he applied to the line a positive pole. They would incidentally assume, however, that he was applying the same current, but, in point of fact, he applied a current in a different direction. For instance, the exposed end of the cable they knew to be copper, and the current set up was a current formed by elements which consisted of the iron sheath of the cable, the water of the sea, and the copper conductor. There would be set up a current of which one part of the cable—the iron sheath—would be the negative pole or positive plate, and another part—the copper conductor—the positive pole or negative plate. Hence it would be seen, at once, what confusion arose from the terms "positive" and "negative" in this case. When they dealt with a cable-current pure, that would be a current set up by the copper of the conductor and the iron of the sheath, and it would flow in a particular direction. The current would be a positive current from the cable. When they applied what they called a positive current, or a copper current, to the conductor, that would be a current which flowed in an opposite direction to the positive current which came from the cable. Here they would see that they at once got into confusion by the use of those two terms "positive" and "negative;" and probably that explanation would enable them to understand more thoroughly the plan which Mr. Fahie adopted, and which he explained at page 6 of his paper. Thus:—"My method, then, consists, briefly, in first eliminating the cable-current, whether it be positive or negative, and then taking, while the cable is free, a simply resistance-test with the bridge, in the manner to be presently described. The cable-current is eliminated by sending into the line a current of the opposite sign to that coming from it, and arranging the strength and duration of this current to suit the strength of the one from the cable." There were variations in the character of currents that were met with in broken cables, and there were variations in the resistance which the ends of broken cables gave. They all knew that there was no point connected with the testing of cables which caused the telegraph engineer so much anxiety and trouble as to account properly for the resistance of the ends. In the first place, they met with earth-currents,—which were currents due to some mysterious causes which they had not yet unravelled. Those currents flowed sometimes in one direction, and sometimes in another; they were very rarely absent from a cable, and whenever a cable was tested they must be eliminated in some way or other. They had, in the second place, those currents of polarisation which were set up by the electrolytic action of the testing current itself, at the end of the cable. In the case which he had supposed, whenever the positive pole was applied to the copper conductor of the cable, the exposed end of the cable decomposed the solution of chloride of sodium, which was the principal constituent of the sea, and caused chlorine to be evolved. The chlorine evolved on the copper would form chloride of copper. In forming that compound it set up a current of polarisation in a direction reverse to the polarising current. When the current was reversed they had sodium evolved on the copper wire; that immediately combined with the oxygen of the water, forming soda, and hydrogen was freed, and consequently they saw bubbles of hydrogen formed upon the copper wire, and these set up currents of polarisation in the reverse direction. Thus, the first series of currents causing disturbance were earth-currents; the second series of currents consisted of currents of polarisation which varied in direction. The third cause of disturbance in testing was the current which was properly called the cable-current, and that was the current which he indicated first as the current set up by the copper of the exposed conductor and the iron sheath. They had also a disturbing effect, due to the variation of the resistance proper of the fault, or the resistance of the material surrounding the fault. If it were a partial fault it would be the resistance of the dielectric or the insulating compound. In the second place, the resistance would be the resistance of the deposit which was formed, whether it was chloride of sodium or some other matter surrounding the fault. These being the causes of disturbance, how were they to be eliminated? Although it was said that these currents of polarisation, and the presence of earth-currents and cable-currents, varied the resistance of the fault, it must be distinctly understood that the variation of resistance was apparent only. The variations appeared because these currents introduced into the measuring apparatus an electromotive force which upset the balance. In the case of a differential galvanometer, for instance, the two measuring-currents were not equal. One current going to line was aided or opposed by another current, due either to an earth-current, to a current of polarisation, or to the cable-current. How was this apparent difference of resistance to be remedied? By far the simplest way to eliminate the error introduced by an earth-current, or a cable-current, was to make the deflection caused by the earth-current the zero of the differential galvanometer, and measure up to that point: this gave exact results. They could get approximately exact results by taking the average of positive and negative measurements taken with the currents varying in direction. Hence, in the case of earth-currents and cable-currents, there was very little necessity to adopt measures like those tried and recommended by Mr. Fahie, or sometimes given in books—such as Sabine's and others—where formulæ of a complicated character were introduced to eliminate these foreign electromotive forces. But when they came to the case of currents of polarisation it was a very different matter indeed, and it was almost impossible to get an exact result unless the operator possessed very great skill and had had a considerable amount of practice. Of all the methods which had been introduced, there was none, as far as he could learn, which gave more exact results than that method which was known as Lumsden's. It was a method based upon a purely scientific fact, and one which, with practice and skill, must lead to proper results. The method was simply this:—When a positive current was sent into a cable the electrolytic action covered the exposed end with chloride of copper. When a negative current was sent into the cable the exposed end was covered with hydrogen. The current of polarisation set up by the chloride of copper was reversed by the current of polarisation set up by the hydrogen. If, therefore, they applied the positive current to a cable, and coated the end with chloride of copper, and then applied a negative current, the hydrogen which was deposited on the other end first of all drove the chloride of copper off, and then it began to form itself. There was an instant when the exposed end itself was in a zero state. Then there was no chloride of copper, and there was no hydrogen, but it was just upon the balance between the two; and Lumsden's method seized upon the exact balance when the cable end was clean, and when it ought to give exact results. Another method was that which had been introduced by Mr. Clark, and in which he (Mr. Preece) had at one time the very great pleasure of acting with Mr. Clark, and assisting him in his experiments. One of the pleasantest times in his telegraphic career was when Mr. Clark and he sat upon the rocks of Jersey, for many hours, working out the effects of the currents of polarisation. It was the first time that currents of polarisation were observed, for the cables were then commencing to break about their heads. The last method to which he should allude was that of cleansing the end: it was described by Mr. Culley in his book, and it was somewhat analogous to the system which Mr. Fahie had really practically discovered; for although this system, and other things which Mr. Fahie had narrated, were to be found in books, it must be remembered that, as far as Mr. Fahie was concerned, he had found them out for himself; and when a man found out a thing for himself he was as much entitled to credit as if he was the first discoverer of it. This last method was, that when there was an earth-current in the cable, the operator set up a current of polarisation of just the proper strength to oppose that current and neutralise it, and in that way he obtained a zero condition of his cable.

Mr. **Alexander Adams** said, with regard to Lumsden's test, that he (Mr. Adams) had found by experiments that the point which Lumsden took as zero was not the zero point, for at that time the copper was really coated with sodium. He had very often tried to get exact results by taking that as the zero, but he had always failed, and generally there had been less resistance than otherwise.

Mr. **Preece** said that Mr. Lumsden's method was simply that between the point when the wire was coated with chloride of sodium and the point when it was coated with hydrogen, there was a zero point, and it was left to the skill of the operator to find exactly where that point was.

Prof. **W. G. Adams**—In testing cables by the Wheatstone's bridge arrangement Mr. Fahie seems to have taken his resistances A and B equal to one another, and to have determined his insulation resistances on the supposition that the resistances of the cable and of his resistance-box R are equal, even when there may be an earth-current or polarisation-current on the cable at the time. This, of course, will not be true, and hence it is the usual plan to create counter-disturbances in the cable to balance the earth-currents —in which case the resistance of cable and fault is equal to the resistance R. When the earth-currents are balanced, or balance one another throughout the cable, and *not only at the testing end*, this will be strictly true; but seeing that there is very great difficulty in bringing the cable to a state of comparative calm, I would ask practical men whether it is not possible to determine the resistance of a cable by measuring the earth-current and comparing it with the current employed to charge the cable? With Mr. Fahie's arrangement, if Q be the current from the battery into the cable, and q an earth-current in the same direction, and r the resistance of the cable, then by Ohm's law—

$$r(Q+q)=RQ,$$

Or—

$$\frac{r}{R}=\frac{Q}{Q+q};$$

q and Q will be directly proportional to the currents at the testing end immediately before and after charging the cable, and so their ratio may be readily measured. I would also ask whether, in practice, the position and resistance of a fault in a line or cable may not be readily found without employing the Wheatstone's bridge method, by determining the strength of currents or the potential at both ends of the line or cable when a charge is given to it? According to Ohm's law, this method should readily settle the question how far the fault is from the end of the cable. The farther off a fault is from the charging station, the less will be the effect of that fault in increasing the amount of the charge given to the cable. But the effect produced at the receiving station will be greatest when the fault is in the middle of the cable; and when a fault of the same resistance is at equal distances from either end of the line, the effects produced at the receiving station will be equal to one another. Whatever be the resistance of a fault, if it occur exactly in the middle of the line, the charges at the two ends are altered by the same amount, so that one is increased by as much as the other is diminished: in this case the mean of the two charges at the two ends of the line is equal to the charge given by the same battery power when the line is perfect.

The **Chairman** said that the formula which took account of the earth-current arising at the other end of a cable was well known, and was published in electrical works, though it was not commonly used in practice—one of the chief reasons that it was not used being that it was almost impossible to get an earth-current to remain steady for two or three minutes together. If it were an absolute cell put on with any power and magnitude, and kept on constantly, there would be no difficulty in eliminating its value, either by the direct measurement of the resistance or by using the formula which Prof. Adams had given; but practically that system was unused, because, as he (the Chairman) supposed, it was not found advantageous. With regard to the paper, the subject which Mr. Fahie had brought forward was one of very great importance to telegraphy. They must all see how important it was, in testing a cable for a fault, for them to know exactly where to lift it. The subject was, therefore, one very well worthy the attention of this meeting. The contribution of Mr. Fahie was a very excellent one, and he (the Chairman) hoped that it would lead others to experiment in the same direction, and lead, sooner or later, to a complete paper on the subject of testing faults, for they had that night but touched on the fringe of the subject. He did not know any more amusing or pleasing experiment than to take a bucket, and make an artificial fault, and watch—with a microscope and a galvanometer simultaneously—the result of putting positive and negative currents on the exposed ends of the copper wire. By a positive current they coated the copper, and increased the resistance; by reversing the polarity they diminished the resistance, and began to form white chloride of copper. By again reversing the current they cleaned off the deposit of chloride of copper, and the bright copper again appeared. These operations might be repeated at pleasure. The experiment was a very interesting one, and one which he should recommend every student of electricity to witness often for himself. There was an intermediate moment when the copper was bright, and when the resistance was at the very lowest point. That point was made use of in what was called Lumsden's method, which was one of the most useful methods of determining the real resistance of the conductor of a cable. He (the Chairman) did not see Mr. Law present at the meeting, and he was sorry for it, for he could have given valuable information on the subject. It surprised him (the Chairman) to hear this method described as Mr. Lumsden's. He did not know at what period Mr. Lumsden introduced it, but certainly as long ago as 1862 he (the Chairman) had seen Mr. Law testing by that method of reversing the currents, and finding out the point of resistance, both practically at sea and also in the laboratory with a microscope, after the manner just described. To his mind Mr. Fahie's plan and propositions were satisfactorily made out, and he thought that Mr. Fahie's system was a more scientific one than even Mr. Lumsden's, or any other method that he was acquainted with. Mr. Fahie, as Mr. Preece had very clearly described, actually brought the cable for a moment into a neutral state, during which—if the observer was quick in taking the resistance—it seemed to him (the Chairman) the resistance could be ascertained more accurately than by any other method. Certainly the plan very closely resembled other plans which were mentioned in the works of Mr. Culley and others, and already well known. In pointing out that the varying resistance caused by using battery powers of different potentials was due not to an actually differing resistance of the fault, but to some change of polarity, Mr. Fahie had made a very useful observation indeed, and he (the Chairman) thought that Mr. Fahie had shown the correct cause of the variation. But it ought not not to be forgotten that that method was a very useful one for determining whether the fault in a cable was a small fault or a large one. Mr. Fahie might have shown that it was an error for him (the Chairman) to state the fact in the words he had used, which were—"The resistance of a *small* fault is much greater with a small battery power, say five cells, than with a higher power, say fifty cells; but if the fault be a *large* one, the resistance will be more nearly equal; and if a greather length of copper be exposed the resistance will be the same." But still he thought that it was a very useful method, and one which might be often resorted to in testing cables. On the whole he was very much pleased with Mr. Fahie's paper. As Mr. Preece had very properly said, it was exactly one of the very class of papers which they were most anxious to obtain. They were desirous to obtain the ideas and experience of persons like Mr. Fahie, who were placed in positions in which they could make practical observations of their own. He felt that this meeting ought to record its sense of the great value of the paper.

A vote of thanks to Mr. Fahie was passed accordingly.

Proceedings of Societies.

SOCIETY OF TELEGRAPH ENGINEERS.

An ordinary meeting was held on Wednesday evening, the 25th ultimo.

Sir William Thomson, F.R.S., President of the Society, took the Chair.

A paper on "*Earth-Boring for Telegraph Poles,*" by Mr. John Gavey, a member of the Society, was read by the Secretary. After the reading of the paper, Mr. Gavey exhibited samples of the instruments which had been described, and explained their use.

There then followed a discussion, in which Messrs. Fischer von Truenfeld, Graves, Bell, Burton, and W. H. Preece, Major Malcolm, Messrs. Culley and Golston, and the President, took part.

A vote of thanks was accorded to the writer of the paper, and the meeting adjourned to the 9th inst.

A summary of the paper, and a report of the discussion, will appear in a future number.

Electrical Science in English and Foreign Journals.

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences, Vol. lxxix., No. 17. October 20, 1874.

On the Conductibility of Indifferently Conducting Bodies.—By the Count du Moncel.—A short paper, in which the author examines the conducting power of tissues, which being more or less hygrometric, should furnish very varying conductivity effects, not only in proportion to the damp state of the atmosphere, but also according to hourly temperature of the day and night. Silks and wools gave far from expected results, for at a relatively low degree of moisture the conductivity of the wools was less than that of the silks. M. du Moncel found invariably that *black* silk gave very large galvanometer readings, whilst other coloured silks scarcely gave any deflections; those that gave the least readings also, curiously enough, cost a higher figure. The conclusions deduced from the experiments are—that, contrary to the generally accepted opinion, the silk stuffs reputed to be insulators are a long way from being so, and that during a period relatively dry woollen stuffs are more generally insulators than silk stuffs—at least for voltaic currents. Colour does not seem to have any well-marked action upon wools. On enquiry into the manufacture of *black* silk, in order to discover the cause of its greater conductibility over other colours, it appears that manufacturers impregnate it with a large quantity of iron and tannin: hence the reason, liquids being much better conductors according as they contain saline materials in solution. And this process is precisely what takes place in the substance of the black silk, through its becoming impregnated with moisture on account of its hygrometric property. Linens most readily absorb atmospheric moisture, and give greatest deflections the greater its coarseness. Cotton goods are likewise large conductors, but to a less degree; the coarser the texture the better the conductivity. The galvanometer indications are so exact for these different sorts of "stuffs" that it is possible to detect the presence of either cotton or thread in silk or woollen materials. The effects of polarisation are less than with minerals, and from carefully-repeated experiments the following conditions were arrived at:—*For a certain direction of the battery current the effects of polarisation gave place to an increase of that current, and that for the contrary direction they determined a considerable and rapid fall.*

A Note on Electro-Magnets.—M. Deleuil.—It was found desirable to substitute for the permanent magnet (used to extract iron particles from the paste or dough of unmanufactured porcelain) an electro-magnet, so arranged that the liquids contained in the paste into which the magnet has to be plunged should not penetrate into the coil. A magnet was devised as follows—A hollow coil was enclosed in a covering of sheet iron whose interior diameter was just large enough to allow the introduction of the coil. The iron paring and chippings contained in the paste are found to attach themselves to it in great abundance, and suits the purpose admirably.

No. 18. November 2, 1874.

Theory of Electro-Dynamics, emancipated from all Hypothesis relative to the Mutual Action of Two Current

Elements.—M. Le Cordier.—A purely mathematical paper, unsuited to our general readers, and best studied in the original.

DIRECT UNITED STATES CABLE COMPANY.

An extraordinary general meeting of the shareholders was held on the 16th ult., at the Cannon Street Hotel —Mr. E. H. Lushington in the Chair—"For the purpose of considering the advisability of raising the necessary funds for the laying of a duplicate cable."

The Chairman said that, before proposing the resolution, he was desirous of stating the present condition of the cable. The first two sections, between Newfoundland and Nova Scotia, and Nova Scotia and the United States, were both laid down, and he knew that they were in excellent working order. With regard to the deep-sea section between Ballinskellig's Bay and Newfoundland, the position was that the *Faraday* had 160 more miles of deep-sea cable to lay, and the *Ambassador* 150 miles of shore cable; therefore he thought they might say that the deep-sea line was practically completed, and the observations he was about to make were based upon the assumption that they had really got one cable laid down, for there was every prospect of the cable being actually laid and in working order in the course of the next few days. Before entering into the subject of the resolution, he wished to call attention to the immense importance of duplicating the Company's deep-sea cable. It was impossible on long lines, at least, to send a message from both ends at the same time, and in the event of a serious accident to their present cable it would take large funds to repair it, and he should be sorry to appear before them under such circumstances, and to ask for financial assistance. There was another fact connected with their cable, namely, that it was laid alongside five other cables belonging to a powerful company, strengthened by monopolies, concessions, and privileges of a certain value. The public were fully alive to the value of these privileges, and it was of the greatest importance that this Company should have every facility for the performance of their duty with the greatest accuracy, punctuality, and regularity, otherwise all but those directly interested in the Direct United States Company would have a natural tendency to go to the Anglo-American Company, where, owing to the number of cables, there would be less chance of anything going wrong with the messages entrusted to them. He was entirely in favour of conducting the business with the utmost prudence, and he ventured to say that extreme prudence would be best shown by agreeing to duplicate the line, and thus putting the Company in that thoroughly safe position in which he was sure they all wished to see it. By the resolution, it was proposed to raise money by debentures, or by whatever means might seem best, to enable the directors to purchase another cable. The directors would take no steps in the matter until they were assured that the first cable was completed and laid, and in good working order. He believed that not more than £5000 per annum additional would be required to work a second cable. He moved the adoption of the following resolution:—"That this meeting, having in view the importance of duplicating the Company's deep-sea cable, and in order to enable the directors to provide the funds for that purpose, and for the general occasions of the Company, resolve, in lieu of section 13 of the article 69 of the articles, which section is hereby abrogated, the following section shall be, and is hereby, substituted, viz.:—No. 13. To raise, from time to time, in the name and for the purposes of the Company, by the issue of debentures upon such terms and conditions as to price of issue, rate of interest, time of repayment, and commutation into shares or stock, and all other respects, as they shall think fit, by way of loan, or in any other manner, any sum or sums not exceeding the present capital of the Company, with power, however, to re-borrow from time to time, in like manner, and within the like limits, for the purpose of redeeming existing debentures or loans, and also to make such payments, by way of commission or otherwise, as they shall think proper, for services with reference to raising any such sums."

Mr. Jos. Sebag seconded the resolution.

An amendment for the adjournment of the meeting for a fortnight was then put and carried.

Mr. C. W. Siemens, in answer to some remarks, said that he would rather not speak on the point whether it was desirable to lay a second cable next year or not, because it might be inferred that he was personally interested in that question. He would therefore, with their permission, confine his remarks to the delay which had occurred in the completion of the work he had in hand. It was true that the first cable was ordered rather before this time last year, and that although—as the Chairman had stated—the cable was virtually completed, yet it was not yet handed over in working order to the Company. These delays were owing to circumstances which he could hardly have foreseen. The cable itself was completed at the date at his works, and the insulation of that cable was not only equal to specification, but about double insulating property to what was stipulated. The insulating resistance stipulated for was 160,000,000 units per knot, and the actual resistance throughout exceeds 300,000,000. The steamship *Faraday*, which was constructed specially to carry out this undertaking, had been a complete success. It had done its work well, and it left within a fortnight of the time originally intended; but when it got out to Newfoundland they were detained by an unusual season of heavy fogs, and in the midst of this difficulty, when the ship had been lost sight of for nearly three weeks, and could not reach any port, and was waiting for clear weather, a report was circulated of the total loss of that vessel. Now, under the circumstances, they were forced to send the attendant ship in search of the *Faraday* to the place where she would most likely be found; but it so happened the *Ambassador* missed the *Faraday*, and the *Faraday* completed her work and came to New Hampshire, and had to wait more than a week for the *Ambassador*. That made a delay of nearly four weeks, and threw them out to such an extent that the chief cable expedition for the section from Ellen's Bay to Newfoundland came virtually into equinoctial gales, which occurred at an unusually early period; about the 10th of September they began to blow, and it was owing to this unfavourable state of the weather that they could not complete the whole laying in one operation. It may be said that the cause of the delay and the breaking away of the end of the cable in a gale of wind was a fault. So it was, but there was no cable ever made without faults. There are faults of such magnitude that speaking or communicating would be impaired thereby, but there was another description of fault which would in no way interfere with the speaking power of the cable, but which electricians wished to avoid. Now, he had said already that the insulation of the cable was 300,000,000, instead of 160,000,000 stipulated in the contract. Perfect gutta-percha, without any defect, can give very little more than that result. However, there is a certain leakage in gutta-percha which must make every inch of the cable to that extent imperfect, and it only depends to what standard you will work to. By the contract, 160,000,000 would have been considered satisfactory, and all the faults would have fallen largely within that limit; but he, acting a little Don Quixotic in the matter in the opinion of some, wished to obtain the highest possible standard he wished to have a cable, and to hand a cable over to this company, which should not be surpassed by any cable in existence. Now, he believed the two sections laid in June were really up to a standard of perfection which had not been surpassed. The insulating resistance, which was fully double what was stipulated in the contract, had been maintained absolutely at that point, and he should not be satisfied unless he handed the main cable over to this company in the same perfect condition. It had only a few faults in the light of a step below the ultimate limit of perfection attainable. Faults or weakness in any particular points of the cable he had given strict instructions to those in charge should not be admitted, and therefore he could hardly admit the charge that this cable was not perfect, and that delays occurred in consequence. It was owing to circumstances beyond their control that a certain delay had taken place, and it was partly due to their desire to attain, as nearly as may be, to the ultimate limit of perfection. Lately they had thought it necessary to cut the end of the cable, and attach it to a buoy in a gale of wind. There was some suspicion that there was a weak point of insulation; there might be a slight injury which the cable had received in paying out, but so slight that the measurement proved an insulation of 300,000,000 units per knot with the fault in it, and therefore the cable was as perfect with this fault in it as it need be for practical purposes, but in the gale the two ships had gone apart. The *Ambassador* had been lost sight of for several days, and the point of rendezvous was Conception Bay, and therefore they had to go there to meet the *Ambassador*, after which they would return to the spot where the cable was buoyed.

Mr. VON CHAUVIN, the general manager of the company, said that Dr. Siemens said that the insulation was twice as good as that stipulated for; but a report he had that morning showed considerably more than that. The insulation of the two sections between New Hampshire and Nova Scotia, and Nova Scotia and Newfoundland, was daily tested by the servants of the company, and reports were made to England weekly. It appeared from those reports and those tests, which were not made by one man, but by various independent operators, that the insulation of the cable was very much better than it need have been. In all the experiments which had been made, or rather the trials made, with the mirror instruments, the speed had been far higher than was anticipated for the present. As far as the lasting power of the cable, as a cable, was concerned, that brought him to the mechanical strength which had been attained in the manufacture of it. He might say a few words, for the satisfaction of the proprietors, as to the mode in which testing had been carried on during the progress of the manufacture. Comparatively, a large staff of electricians had been appointed by the company, and it was the duty of these electricians to test, not only daily and hourly, but to test almost from five minutes to five minutes every piece of cable under process of manufacture, and every piece of cable which had been manufactured, and to test also the raw materials which were supposed to be employed for the manufacture of the cable. They had introduced a mode of testing which hitherto had not been introduced. They tested the various coats of gutta-percha; they tested the copper wire continuity and flexibility; they tested the manilla hemp; they tested the steel wire for flexibility; and they found that, whereas it was specified to bear 53 tons per square inch, it only broke at 56. He had no hesitation in saying, as far as he could form an opinion, and to a certain extent, as an engineer, he ought to be able to do, that this cable would last a considerable time—perhaps a longer time than many other cables he had seen laid. He might also say that while these tests were being carried on by the company, independent tests were carried on by Messrs. Siemens, and he was allowed to check their results. Whatever had been actually laid was far superior to that which they had a right by the contract to expect, and far superior to the usual standard of cables. As to the working speed which they were likely to attain, although that was a matter on which he could not speak with absolute certainty, he might mention one thing—that, owing to a different construction of the copper conductor of the cable, the transmitting power of the cable had been increased, and he thought that they would have even a higher speed of working than the same weights of copper and gutta-percha would have given under the ordinary construction.

A cordial vote of thanks to the Chairman, proposed by Mr. BRISTOWE, and seconded by Mr. COHEN, terminated the proceedings.

A COMMUNICATION has been received from the *Faraday* steamer, stating that the operations in connection with the Direct United States Cable had been delayed by bad weather. The cable had been taken up, but was to be buoyed again, owing to the continuance of the gale.

THE liquidators of the British-Indian Submarine Telegraph Company, Limited, the Anglo-Mediterranean Telegraph Company, Limited, and the Falmouth, Gibraltar, and Malta Telegraph Company, Limited, have issued their final report for presentation to the meeting on the 3rd proximo. They state that all the concessions, property, and liabilities of the companies have been transferred to the Eastern Telegraph Company, Limited, and that the undertakings are fully wound up in accordance with the terms of the resolution of the 3rd October and 1st November, 1872.

AN important extension of the application of electricity as an illuminating agent is, we understand, about to be made by the Trinity Board. The two lighthouses on the Lizard Point are to be fitted with the requisite apparatus for the production of the electric light. The penetrating power of this light will be of the utmost value at this, the first point of land made by homeward-bound ships.—*Athenæum*.

TELEGRAPH SHARE LIST.

Amount per Share.	NAME OF COMPANY.	Amount paid up.	Closing Quotations.
£		£	Nov. 30.
Stock	Anglo-American (Limited)	100	73—73½
10	Brazilian Submarine..	All	7¼—7¾
10	Cuba	All	5¾—6¼
10	Direct Spanish	9	..—..
20	Direct United States Cable	All	11½—12½
10	Eastern (Limited)	All	7⅜—7⅝
..	Do., New	..	103—105
10	Eastern Extn. Australia and China	All	7⅜—7⅝
10	Globe Telegraph and Trust	All	6½—6¾
10	Do., 6 per cent Pref...	All	9¾—10
10	Great Northern	All	11—11½
25	Indo-European	All	17—18
10	Mediterranean Extension (Limited)	All	3¾—4¼
10	Do., 8 per cent Pref...	All	10½—11
10	Panama and South Pacific	2½	..—..dis
8	Reuter's..	All	11—11½
Stock	Submarine	100	206—211
1	Do., Scrip	All	2—2¼
10	West India and Panama	All	3⅝—3⅞
10	Do., 10 per cent Pref.	All	8¾—9¼
20	Western and Brazilian (Limited) ..	All	11½—12
1000 dls.	West Un. U.S. 7 per cent 1st M.B.	All	102—104
10	Hooper's Telegraph Works	All	12¾—13¼
50	India-Rubber and Gutta Percha ..	All	21—23
Cert.	Submarine Cables Trust	100	106—109,
12	Telegraph Construction	All	27—28
100	Ditto Ditto 7 per cent Bonds	All	103—105.

To Correspondents.

We have received a communication from Mr. D., of Halifax, on an anomalous effect observed on magnetising or demagnetising a steel needle. We have repeated Mr. D.'s experiment, but are unable to confirm the results he has obtained. Will our correspondent verify his observation, and send us his result? In reply to another communication from Mr. D., we hope in an early number to give full particulars of the construction and mode of using the instrument in the manipulation of which he has failed (with many others) to secure good results.